terrestrial magnetism would be derived from a new magnetic survey of the southern hemisphere extending from the parallel of 40° S. as far towards the geographical pole as possible."

Intimately connected with terrestrial magnetism are the phenomena of auroras. Their nature is very obscure, but quite recently a distinct advance has been made towards discovering some of the laws which regulate them. Thanks to the labours of Dr. Sophus Tromholt, who has spent a year within the Arctic circle studying them, we now know that their movements are not as eccentric as they have hitherto appeared to be. He tells us that the Aurora Borealis, with its crown of many lights, encircles the Pole obliquely, and that it has its lower edge suspended above the earth at a height of from 50 to 100 miles, the mean of 18 trigonometrical measurements, taken with a base line of 50 miles, being 75 miles. The aurora forms a ring round the Pole which changes its latitude four times a year. At the equinoxes it attains its greatest distance from the Pole, and at midsummer and midwinter it approaches it most closely, and it has a zone of maximum intensity which is placed obliquely between the parallels of 60° and 70° N. The length of its meridional excursion varies from year to year, decreasing and increasing through tolerably regular periods, and reaching a maximum about every eleven years, when, also, its appearance simultaneously attains to its greatest brilliancy. Again, it has its regular yearly and daily movements or periods. At the winter solstice it reaches its maximum annual intensity, and it has its daily maximum at from 8 p.m. and 2 a.m., according to the latitude. Thus at Prague, in lat. 50° N., the lights appear at about 8.45 p.m.; at Upsala, lat. 60° N., at 9.30 p.m.; at Bossekop, 70° N., at 1.30 a.m. Now, while these data may be true for the northern hemisphere, it remains to be proved how far they apply to the southern. Indeed, seeing that the atmosphere of the latter region is moister and shallower than that of the former, it is probable that the phenomena would be modified. A systematic observation of the Aurora Australis at a number of stations in high latitudes is therefore desirable.

Whether or not there is any connection between auroral exhibitions and the weather is a disputed point. Tromholt believes that such a relationship is probable ("Under the Rays," 1283). He says that, "however clear the sky, it always became overcast immediately after a vivid exhibition, and it generally cleared again as quickly" ("Under the Rays," 1235). Payer declares that brilliant auroras were generally succeeded by bad weather ("Voyage of Tegelhoff," 1324), but that those which had a low altitude and little mobility appeared to precede calms. Ross remarks of a particular display "that it was followed by a fall of snow, as usual" ("Ross's Voyage," 1312). Scoresby appears to have formed the opinion that there is a relationship indicated by his experience. It is, therefore, allowable to regard the ultimate establishment of some connection between these two phenomena as a possible contingency. If, then, we look at the eleven-year cycle of auroral intensity from the meteorological point of view, it assumes a new interest, for these periods may coincide with the cycles of wet and dry seasons, which some meteorologists have deduced from the records of our Australian climate, and the culmination of the one might be related to some equivalent change in the other. For if a solitary auroral display be followed by a lowered sky, surely a period of continuous auroras might give rise to a period of continuous cloudy weather, with rain and snow. Fritz considers that he has established this eleven-year cycle upon the strength of auroral records extending from 1583 to 1874, and his deductions have been verified by others.

In January 1886 we had a wide-spread and heavy rainfall, and also an auroral display seen only at Hobart, but which was sufficiently powerful to totally suspend communication over all the telegraph lines situated between Tasmania and the China coast. This sensitiveness upon the part of the electric currents to auroral excitation is not novel, for long experience on the telegraph wires of Scandinavia has shown that there is such a delicate sympathy between them that the electric wires there manifest the same daily and yearly periods of activity as those that mark the auroras. The current that reveals itself in fire in the higher regions of the atmosphere is precisely the same current that plagues the operator in his office. Therefore, in the records of these troublesome earth-currents, now being accumulated at the Observatory by Mr. Ellery, we are collecting valuable data, which may possibly enable the physicist to count the unseen auroras of the Antarctic, to calculate their periods of activity and lethargy, and, again, to check these with our seasons. But it need hardly be said that the observations, which may be made in the higher latitudes and directly under the rays of the Aurora Australis, will have the greater value, because it is only near the zone of maximum auroral intensity that the phenomena are manifested in all their aspects. In this periodicity of the southern aurora I have named the last scientific problem to which I had to direct your attention, and I would point out that if its determination should give to us any clue to the changes in the Australian seasons which would enable us to forecast their mutations in any degree, it would give to us, in conducting those great interests of the country which depend for their success upon the annual rainfall, an advantage which would be worth, many times over, all the cost of the expeditions necessary to establish it.

Finally, there is a commercial object to be served by Antarctic exploration, and it is to be found in the establishment of a whaling trade between this region and Australia. The price of whalebone has now risen to the large sum of £2000 a ton, which adds greatly to the possibilities of securing to the whalers a profitable return. Sir James Ross and his officers have left it on record that the whale of commerce was seen by them in these seas, beyond the possibility of a mistake. They have stated that the animals were large, and very tame, and that they could have been caught in large numbers. Within the last few years whales have been getting very scarce in the Arctic, and in consequence of this two of the most successful of the whaling masters of the present day, Captains David and John Gray, of Peterhead, Scotland, have devoted some labour to collecting all the data relating to this question, and they have consulted such survivors of Ross's expedition as are still available. They have published the results of their investigations in a pamphlet, in which they urge the establishment of the fishery strongly, and they state their conclusions in the following words. They say:—"We think it is established beyond doubt that whales of a species similar to the right or Greenland whale, found in high northern latitudes, exist in great numbers in the Antarctic seas, and that the establishment of a whale fishery within that area would be attended with successful and profitable results." It is not necessary for me to add anything to the opinion of such experts in the business. All I desire to say is that if such a fishery were created, with its head-quarters in Melbourne, it would probably be a material addition to our prosperity, and it would soon increase our population by causing the families of the hardy seamen who would man the fleet to remove from their homes in Shetland and Orkney and the Scotch coasts, and settle here.

In conclusion, I venture to submit that I have been able to point to good and substantial objects, both scientific and commercial, to justify a renewal of Antarctic research, and I feel assured that nothing could bring to us greater distinction in the eyes of the whole civilized world than such an expedition, judiciously planned and skilfully carried out.

QUARTZ FIBRES.[1]

BEFORE I enter upon the subject upon which I have to address you, I wish to point out that, quite apart from any deficiency on my part which will be only too apparent in the course of the evening, it is my intention to commit two faults which may well be considered unpardonable. In the first place, I shall speak entirely about my own experiments, even though I know that the iteration of the first personal pronoun for the space of one hour is apt to be as monotonous to an audience as it is wanting in taste on the part of a lecturer. In the second place, I am going almost to depend upon the motions of a spot of light to illustrate the actions which I shall have to describe, in spite of the fact that it is impossible for an audience to get up any enthusiasm when watching the wandering motion of a spot of light the result of the manipulation of a mystery-box, of which it is impossible to see the inside. These, however, are faults which are the immediate consequence of the nature of my subject.

Physicists deal very largely with the measurement of extremely minute forces, which it is of the utmost importance that they should be able to measure accurately. Now forces may be considered under two aspects. It may be that the force which is developed and which has to be measured is a twist, in which case the twisting force may be applied to the end of a wire directly, when the amount through which that wire is twisted is a measure of the twisting force. Or the force may be a direct pull or a push, which may also be measured by the twist of a wire if it is applied to the end of a lever or arm carried by the wire.

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[1] Lecture delivered by Prof. C. Vernon Boys, F.R.S., on September 8, 1890, at the Leeds meeting of the British Association.

Now supposing that the force—whether of the nature of a twist or of a pull, it does not matter which—is too small to produce an appreciable twist in the wire, it is obvious that a finer wire must be employed, but it is not obvious how much more easily a fine wire is twisted than a coarse one. If the fine wire is one-tenth of the diameter of the coarse one, we must multiply ten by itself four times over in order to find how much more easily twisted it is, and thus obtain the enormous number 10,000; it is 10,000 times more easily twisted than the coarse one. Thus there is an enormous advantage in increasing the minuteness of the wire by means of which feeble twisting or pulling forces are measured. But if the delicacy of the research is such that even the finest wire which can be made is still too stiff, then, even though with such wire, which is somewhere about the thousandth of an inch in diameter, forces as small as the millionth part of the weight of a single grain can be detected with certainty, the wire is of no use; and as wire cannot be made finer, some other material must be used. Spun glass is fine and strong, and is still more easily twisted than the finest wire, but it possesses a property somewhat analogous to putty. When it has been twisted and then let go, it does not come back to its old place, so that though it is much more largely twisted than wire by the application of a force, it is not possible with accuracy to measure that force. There is, or rather I should say there was, no material that could be used as a torsion thread finer than spun glass; and therefore physicists use instead a fibre almost free from torsion. A single thread of silk as spun by the silkworm is taken and split down the middle, for it is really double, and one half only is used. This is far finer than spun glass, and being softer in texture, it is much more easily twisted. Silk is ten thousand times more easily twisted than spun glass. So easily twisted is silk that in the majority of instruments the stiffness of the silk is either of no consequence at all, or at any rate it only produces but the slightest disturbing effect. Now if it is necessary to push the investigation further still by the continued increase in the delicacy of the apparatus, silk itself begins to prevent any progress. Silk has a certain stiffness, but if that were always the same it would not matter; but then it possesses that putty-like character of spun glass, but in a far higher degree; it is affected by every variation of temperature and moisture, and any really delicate measures are out of the question when silk is used as the suspending fibre.

This, I believe, is a fairly accurate account of the state of the case three years ago. At that time I was improving, or attempting to improve, a certain class of apparatus of which I shall have more to say presently, and I was met by the difficulty that a greater degree of delicacy was required than was possible with existing torsion threads. Silk would have entirely prevented me from reaching the degree of delicacy and certainty in this instrument that I hope to show this evening that I have attained.

Being then in this difficulty, I was by good fortune and necessity led to devise a process which I propose at once to show you. I shall not describe the preliminary experiments, but simply describe the process as it stands. There is a small cross-bow held in a vice, and a little arrow made of straw with a needle point, and I have here a fragment of rock crystal which has been melted and drawn into a rod. It requires a temperature greater than that developed in any furnace to melt this material so that it may be drawn out. If the arrow, which also carries a piece of the quartz rod, is placed in the bow, and if both pieces are heated up to the melting-point and joined together, and then the arrow is shot, a fibre of quartz is drawn—that is to say, it is drawn if there is not an accident.

The arrow has flown, and there is now a fibre, not very fine this time, which I shall hand to our President. At the same time I can pass him a piece of much finer fibre, made this afternoon, which shows (and this is a proof of its fineness) all the brilliant colours of the spider line when the sun shines upon it, but with a degree of magnificence and splendour which has never been seen on any natural object.

The main features of these fibres are these. You can make them as fine as you please; you can make them of very considerable length; you can make pieces 40 or 50 feet long, without the slightest trouble, at almost every shot. Even though of that great length, they are very uniform in diameter from end to end, or, at any rate, the variation is small and perfectly regular. The strength of the fibre is, I think I may safely say, something astonishing. Fibres such as I have in use at the present time in an instrument behind me are stronger than ordinary bar steel: they carry from 60 to 80 tons to the square inch. That is one of their most important features, for this reason—that on account of their enormous strength you can make use of very much finer fibres than would be possible if they were not so strong; and I have already explained the importance of the fineness of the fibre when delicacy is of the first importance.

As to the diameter of these fibres, I have said they can be made as fine as you please. I shall not trouble you with a large number of figures, but one or two may probably be interesting to those who are in the habit of using philosophical apparatus. In the first place, a fibre a great deal finer than a single fibre of silk—that is, one five-thousandth of an inch in diameter—will carry an apparatus more than thirty grains in weight. I have in one of the pieces of apparatus which I shall use presently a fibre the fifteen-thousandth of an inch in diameter. That is, so fine that if you were to take a hundred of them and twist them into a bundle you would produce a compound cable of the thickness of a single silkworm's thread. I do not mean the silk used for sewing that is wound on a reel, because that is composed of an enormous number of silk threads; but a single silkworm's thread as it is wound from the cocoon, and that fibre is at the present time carrying a mirror the movements of which will presently be visible in all parts of this large room.

But that is by no means the limit of the degree of fineness which can be reached. A fibre the fifteen-thousandth of an inch in thickness is quite a strong and conspicuous object. You may go on making them until you cannot see them with the naked eye. You may go on following them with the microscope until you cannot see them with the microscope—that is to say, you cannot find their end—they gradually go out. The ends are so fine that it is impossible ever to see them in any microscope that can be constructed, not because the microscopes are bad, but because of the nature of light. But that is a point upon which I shall not say more this evening. It has been estimated that probably the ends of some of these are as fine as the millionth part of an inch—I do not care whether they are or whether they are not, because they can never be seen and never be used—but certainly the hundred-thousandth of an inch is by no means beyond the limit which can be obtained. As these large numbers of hundreds of thousands and millions are figures which it is impossible for anybody thoroughly to realize, I may for the purpose of illustration say that, if we were to take a piece of quartz about as big as a walnut, and if we could draw the whole of that into a thread one hundred-thousandth of an inch in diameter—threads which can certainly be produced—there would be enough to go round the world about six or seven times.

These quartz fibres, on account of their fineness, are eminently capable of measuring minute forces—that is to say, they would be capable if they were free from that putty-like quality which I have described as making spun glass useless. Now, experiments made both in this country and in Australia show that to a most extraordinary degree they are perfectly free from that one fault of spun glass.

The number of useful properties of quartz that has been melted is so great that I can merely take, in a more or less disjointed way, one or two; and I propose, in the first place, to say something which, I think, may be especially interesting to chemists, and, perhaps, to our President. I should like to ask experimental chemists what they would think of a material which could be drawn into tubes, blown into bulbs, joined together in the same way that glass is joined, drawn out, attached to a Sprengel pump, sealed off with a Sprengel vacuum, which would be transparent, which would be less acted upon than glass by corrosive chemicals, and which, finally, at the point at which platinum is as fluid as water, would still retain its form. Here is such a tube with a bulb blown at the end. I have found that it is possible to make tubes (though it cannot be done in the ordinary way, as with glass) and to blow bulbs with quartz, and that they have this advantage which glass does not possess—namely, that it is almost impossible to crack them by the sudden application of heat.

Then there is another property which quartz fibres and rods possess which I shall be able to show only imperfectly—namely, the power of insulating anything charged with electricity under conditions under which in general insulation is impossible. You now see upon the screen an electroscope, the leaves of which were charged at noon, and they are still divergent, but not to a very great extent, because they have suffered from unavoidable shaking during the day. The point to which I specially wish to refer is this. In electroscopes and all electrostatic apparatus one puts in a dish of sulphuric acid, which is an abomination, in order to keep the atmosphere dry. I have in this electroscope such a dish, but it is filled with water in order to keep the atmosphere moist. Experiments carefully made, using the same box—everything the same—except that in one case the insulating stem was made of quartz, and in the second case it was made of the best flint glass, well washed, of the same shape and size, show that, if the atmosphere is perfectly dry, the electricity escapes from both at the same rate; but that, if the atmosphere is perfectly moist, the electricity escapes from the leaves insulated by the clean-washed flint glass only too quickly; whereas, from the leaves insulated by the quartz, the rate is identically the same as it was in either case when the atmosphere was perfectly dry.

I have said that these fibres are uniform in diameter, and fine and smooth and strong, and that they glisten with all the colours of the spider web, but that they are far more brilliant. It was naturally rather a curious point to note what a spider would do if by any chance she should find herself on such a web, and now that I am dealing with live and wild animals which cannot possibly be trained the conditions are such as to render the success of an experiment entirely a matter of chance. However, I propose to make use of the spider as a test of the very great smoothness and slipperiness of one of these fibres. There are here three little spiders which have been good enough, since they came to Leeds, to spin upon these little wooden frames their perfect and beautiful geometrical webs. I have succeeded in placing one of these frames in the lantern without disturbing the spider, which you can now see waiting upon her web. I must now, without disturbing the peace of mind of the spider, carry her to a web of quartz; and therefore it is necessary that the spider should be fortunate enough to catch a fly. Now, instead of bringing a fly I will make an ordinary tuning-fork buzz against the web. She immediately pounces upon the imaginary fly, and thus I can without frightening her place her upon the quartz fibre. Unfortunately this spider has slipped and has got away, but with another I am more successful. I intended to show that the small and common garden spider could not climb the quartz fibre, but for some reason this spider is able to get up with difficulty; however I shall not spend any more time upon this experiment.

I shall now at once speak about the instrument which actually led me to the invention of the process for making quartz fibres. This, which I have called a radio-micrometer, is an instrument of very great delicacy for measuring radiant heat from such a thing as a candle, a fire, the sun, or anything else which radiates heat through space.

The radio-micrometer which I wish to show this evening is resting upon a solid and steady beam, and as usual its index is a spot of light upon the scale. You see that that spot of light is almost perfectly steady. Now the heat that I propose to measure, or rather the influence of which I intend to show you, is the heat which is being radiated from a candle fixed in the front of the upper gallery some 70 or 80 feet from the instrument; and in order that you may be sure that the indication of the instrument is due to the heat from the candle, and not to any manipulation of the apparatus on the beam, I shall perform the experiment as follows. None of the apparatus at this end of the room will be touched or moved in any way; but by a string I shall simply pull the candle along a slide up to a stop, at which position it will shine upon the sensitive part of the radio-micrometer. Instantly the spot of light darts along the scale for a distance of ten feet, and then after leaving the scale it comes to rest upon the face of the balcony five or six seconds after it began to move. Now if the candle is allowed to move back through about a foot, you will see that the instrument will cool down at once—it is at present suffering from the heat which falls upon it from the distant candle; but it will cool down at once, and the index will go back to its old place. It is very nearly at its old place now. I will now let the candle shine upon it again. The index at once goes on to the balcony as before, and now that the candle is moved away again, the index has assumed its old place upon the scale.

That really shows that we have here the means of measuring heat with a degree of delicacy, and also with a degree of certainty, ease, and quickness, which has never yet been equalled. It is probable that the measure which I have given of the degree of delicacy that I have reached in my astronomical apparatus—namely, that the heat of a candle more than two miles away can certainly be felt—will not seem so absurd now that you have seen this less perfect apparatus at work, as it does to people whose experience is limited by the thermopile or their senses.

You can now see the spot of light; it is perfectly quiet in its old place. I wish to show you that this instrument is unlike those which are ordinarily used for this purpose. All the heat, the very considerable heat, due to this electric arc lamp, is actually falling on the instrument, but not upon its sensitive surface, and there is no indication. There are a large number of people in the room—it does not feel the heat from them. Stray heat which it is not meant to feel—which is not in the line along which it can see, or feel—has no influence upon it. When the candle was moved to the place to which it was looking, it felt the heat, and you saw the movement of the index. What is perhaps more important than all is, that it is an instrument which does not even feel the influence of a magnet. I have here a magnet, and on waving the magnet about near the instrument there is no movement of the index at all; it does not dance up and down the scale, as it certainly would do in the case of a galvanometer, because this magnet would affect a galvanometer at the other end of the room. We have then a degree of sensibility which is certainly not easily developed in any other way. I must except, however, the instrument which Prof. Langley of America has recently brought to a great state of perfection. I am unable to state, from want of information, whether his instrument is as sensitive as the one I have just shown, but whether it is or is not as sensitive it certainly cannot compare with this in its freedom from the disturbing effects of stray heat falling upon it, or of the magnetic or thermo-electric disturbances which give so much trouble where the galvanometer is employed.

Now this apparatus I was recently using in some astronomical experiments on the heat of the moon and the stars. As these experiments could only be made with an instrument such as this, possessing extreme sensibility and freedom from extraneous disturbances, and as this instrument is both the cause of the discovery and the first result of the application of quartz fibres, I have thought it well to repeat a typical experiment upon the moon's heat, but, like Peter Quince, I am in this difficulty. As he said, "There is two hard things, that is to bring the moonlight into a chamber." In fact, at the present time the moon has not risen, and if it had we should not be much better off. Peter Quince proposed that they should in case of moonlight failing have a lanthorn and a bunch of thorns. That no doubt was sufficient for the conversation of Pyramus and Thisbe, but that would not do for the purpose of showing the variation of radiation from point to point upon the moon's surface, and as that is the experiment which I now wish to show—an experiment which this instrument enables one to make with the greatest ease and certainty—it is necessary to have something better than a lanthorn and a bunch of thorns. Therefore I have been obliged, as the moon is not available, to bring a moon. Now this moon is a real moon; it is not a representation; it is not a slide; it is a real moon, and it is made by taking an egg-shell and painting it white. That egg-shell is now placed upon a stand, and is illuminated by the sun—that is, an electric light—and in order that the moon may be visible the room must be darkened. The moon is now shining in the sky. An image of the moon is cast by means of a concave mirror upon a translucent screen. There is in addition another mirror which throws a small image of the same moon upon the radio-micrometer. There is one more thing to explain. There is upon the screen a black spot which represents the sensitive surface of the radio-micrometer. That bears the same proportion to the moon which you see on the screen as the sensitive surface of the radio-micrometer bears to the image of the moon that is cast upon it. Now the two mirrors are arranged to move by clockwork, so as to make the two images travel at proportional rates. The moon is travelling with the dark edge foremost, and now that the terminator of the moon has come upon the sensitive surface, the heat is felt and the deflection of the instrument is the result. Now as the moon is gradually travelling through the sky, the radiation is slowly and steadily increasing, because the radiation from the moon gets greater and greater, as the point at which the sun is shining vertically—that is, a point at right angles with the terminator—is approached; it is here a maximum, and then it falls back, and as soon as the moon has gone off the instrument, you will see the index fall back almost suddenly. But there is something more. This moon in one respect is better than the other moon. At the present time it represents the moon nineteen days old, a moon, that is to say, which is waning, and which goes through the sky with its dark edge foremost. The clockwork will now bring the moon back again, and convert the nineteen-day moon into a nine-day moon, one in which the bright edge goes forward. What I want you to notice, and it will be perfectly evident, is this, that the spot of light will now go up the scale suddenly, will then rise to a maximum position, and will then fall slowly until the terminator is reached, which proves that in the former case the slow rise and sudden fall, or the present sudden rise and slow fall was not a peculiarity of the instrument, but was due to the fact that the different points of the moon radiated in the manner which I have stated. There is one point which, as the moon has now left the instrument, I should like to show; that is, that it is a real moon and not a mere slide. That is shown by gradually moving the sun round. Now it is at right angles to the line of view, and we have got the half moon. As it goes round, the moon continues waning, appearing more like a new moon, and at last we have an eclipse of the sun, which may be annular if the proportions of the apparatus are properly arranged.

I wish now to make a few statements as to the delicacy of apparatus that can be made with the help of quartz fibres. I would wish you most distinctly to understand that it is not sufficient to go into a shop and buy apparatus as it is now made, replace the silk by quartz, and to suppose you can get a degree of delicacy such as I have shown you. That is not sufficient. If you take out the silk and put in a quartz fibre the apparatus will be much improved, and you can then increase its delicacy. You will then escape the troubles due to silk; but one after the other a new series of disturbances will appear, and anything like ultimate, extreme and minute accuracy will still seem out of the question. Now it has been my business to eliminate one by one these disturbing influences. I will not weary you with a description of them all, and the methods by which they may be certainly provided against. These disturbing causes, which at the present time with instruments carrying a silk fibre are not even known to exist, or if known to exist, are practically of no consequence whatever, come one by one into prominence, when you attempt to push the delicacy of your apparatus to the extent that I have reached in the home-made apparatus which I have here this evening. I do not propose to give more than one illustration, and as this is one which I found out by accident, and which at the time very much annoyed me, I imagine that it may be of interest to explain the circumstances under which this was observed.

In the experiments I made on the heat of the moon and the stars it was necessary to determine to what degree of delicacy the apparatus could be brought—that is to say, to determine what deflection would be produced by a known and familiar source of radiation. For this purpose the source of heat that I used was a common candle, placed sufficiently far off to produce a convenient deflection. I began by placing the candle about 100 yards away, but I was obliged to place the candle at a distance of 250 yards. At that distance I could not conveniently at night turn the shutter on and off with a string. Therefore I adopted the more simple and practical plan of asking my niece to stand at the top of the hill and to pull the string when I gave the signal. The signal was nothing more nor less than my saying the word "on" or "off," so that without moving I could observe the deflection due to the heat of the candle at that distance. Those were the circumstances, but when I shouted "on," before the sound could have reached my niece at the top of the hill, the spot of light had been driven violently off the scale. This seemed as if, as I suspected at the time, one of my little eight-legged friends had got inside the apparatus, and feeling the trembling due to the sound, struck forward, as the diadema spider is known to do, and tried to catch the thing that was flying by. But further experiments showed that this was not the case. It happened that the sound of my voice was just that to which the telescope tube would respond. It echoed to that note, the instrument felt the vibration of the air, and that was the result.

In order to show that an instrument will feel the motion in the air under the influence of sound, I have arranged an experiment of the simplest possible character. I should say that the first instrument of this kind was made many years ago by Lord Rayleigh; but I feel sure that even he would not be prepared for the delicacy to which apparatus on this principle can be brought. It simply depends upon this familiar and well-known fact. A card or a leaf allowed to drop through the air does not fall the way of the least resistance—that is, edgeways—but it turns into the position of greatest resistance, and falls broadside on, or it overshoots the mark, and so gets up a spin.

Supposing you take a little mirror suspended at an angle of 45° to the direction of the waves of sound, the instant sound-waves proceed to travel, that mirror turns so as to get into such a position as to obstruct them. The mirror that I have for this purpose weighs about the twentieth part of a grain, and the fibre on which it is suspended is about the fifteen-thousandth part of an inch in diameter. The mirror is so small and light that the moment of inertia is a two-hundredth part of that which people ordinarily call the minute and delicate needle of the Thomson mirror-galvanometer. With a fibre only a few inches long, there is no difficulty in getting a period of oscillation of ten or eleven seconds. When the light from the lamp is reflected and falls upon the scale, as it will be in a minute, then a movement of the light from one of those great divisions to the next—that is, a movement of three inches—will correspond to a twisting force such as would be produced by pulling the end of a lever an inch long with a force of a thousand-millionth part of the weight of a grain. It would be easy to observe a movement ten or a hundred times less. My difficulty now is that it is impossible to speak and at the same time to keep that spot at rest, because the instrument is arranged to respond to a certain note. This is not the predominating note of my voice, but since the voice, like all other noises as distinguished from pure musical sounds, consists of a great number of notes, every now and then the note to which the instrument is tuned is sure to be sounded, and then it will respond. Therefore, while I am speaking it is impossible to keep the spot of light at rest. However, in order to show that the instrument does respond to certain notes, even if feeble, with a degree of energy and suddenness which I believe would never be expected, I shall with these small organ pipes sound three notes. But I must explain beforehand what I am going to do, as the sound of my voice will spoil the experiment. I shall, standing as far away as I can get from the instrument, first sound a note that is too high; I shall then sound a note that is too low; and then I shall sound the note to which the instrument is tuned. I must ask everyone during this experiment to be as quiet as possible, as the faintest sound of the right sort will interfere with the success of the experiment. The first two notes sounded loudly produced no result, while the moment the right note was heard the light went violently off the scale and travelled round the room. When this little organ pipe was blown at the farthest end of the room this afternoon, it drove the light off the scale, almost as violently as it did just now.

[The Cavendish experiment of observing the attraction due to gravitation between masses of lead was then explained; and the actual experiment, performed with apparatus no larger than a galvanometer, in which the attracting masses were two pounds and fifteen grains respectively, in which the beam was only about five-eighths of an inch long, and in which the total force was less than one ten-millionth of the weight of a grain, was then shown. The actual deflection on the scale was rather more than ten feet, and eighty seconds were required for the single oscillation. With this apparatus forces two thousand times as small could be observed, though the fibre is, in comparison with others that were made use of, exceedingly coarse. Forces equivalent to one million-millionth of the weight of a grain were stated to be within the reach of a manageable quartz fibre.]

Now that I have shown all that my limited time has permitted me, I wish finally to answer a question which is frequently put to me, and which possibly some in the room may have asked themselves. The question may be put broadly in this form: "These fibres no doubt are very fine, and very wonderful, but are they of any practical use?" This is a question which I find it difficult to answer, because I do not clearly know what is meant by "practical use." If by "a thing of practical use" you mean something which is good to eat or to drink, or if you mean something which we may employ to protect ourselves from the extremes of heat or cold or moisture, or if you mean —and this is a point which those who have studied biology will perhaps appreciate more than others—something which may be made use of for the purpose of personal adornment; if that is what you mean by "practical use," then with the exception of the possibility of being able to weave garments of an extraordinary degree of fineness, softness, and transparency, quartz fibres are of no "practical use." But if you mean something which will enable a large and distinguished body of men to do that which is most important to them more perfectly than has been possible hitherto—I allude of course to the experimental philosopher and his experimental work, which after all has laid the foundations upon which so much that is called practical actually is built—if this is what you mean, then I hope that the few experiments which I have been able to show this evening are sufficient to prove that quartz fibres are of some practical use; and they have served this additional purpose—with what success I am unable to say—they have provided a subject for an evening lecture of the British Association.

SOCIETIES AND ACADEMIES.

London.

Entomological Society, October 1.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—The Rev. Dr. Walker exhibited, and read notes on, a long and varied series of *Crymodes exulis*, collected in June and July last in Iceland. In reply to a question by Lord Walsingham as to whether all the forms referred by Dr. Walker to *Crymodes exulis* had been identified as belonging to that species, Mr. Kirby said the species was a very variable one, and that several forms had been described from Labrador and Greenland. Mr. South stated he believed that most of the forms had been described by Dr. Staudinger.—Dr. D. Sharp, F.R.S., exhibited a specimen of *Ornithomyia avicularia*, L., taken near Dartford, to which there were firmly adhering—apparently by their mandibles—several specimens of a mallophagous insect. He also exhibited specimens of fragile Diptera, Neuroptera, and Lepidoptera, to show that the terminal segments in both sexes might be dissected off and mounted separately without the structures suffering from shrivelling or distortion.—Mr. G. F. Hampson exhibited a series of *Erebia melas*, taken in July last, in the Austrian Alps (Dolomites), by Mrs. Nicholls. Captain Elwes observed that this species was abundant in the Pyrenees, but he had never been able to obtain specimens from any other part of Europe; and that it had been left to an English lady to first take a species of *Erebia* new to these Alps. He added that the species only frequented very steep and stony slopes on the mountains, so that its capture was attended with difficulty.—Mr. McLachlan, F.R.S., exhibited specimens of an extraordinary Neuropterous larva found by Mr. B. G. Nevinson in tombs at Cairo. He said that this larva had been assigned to the genus *Nemoptera* by Schaum, and Roux had previously described and figured it as an abnormal apterous hexapod under the name of *Necrophilus arenarius*. Mr. Nevinson supplemented these remarks with an account of his capture of the specimens in the Egyptian tombs.—Mr. G. T. Baker exhibited species of the genus *Boarmia* from Madeira; and also melanic varieties of *Gracilaria syringella* from the neighbourhood of Birmingham.—Mr. W. F. H. Blandford exhibited and remarked on specimens of *Dermestes vulpinus*, a wood-boring beetle, which had been doing much damage to the roofs of certain soap-works in the neighbourhood of London.—Mr. R. W. Lloyd exhibited a specimen of *Carabus catenulatus*, in which the femur of the right foreleg was curiously dilated and toothed.—The Rev. C. F. Thornewill exhibited a black variety of the male of *Argynnis aglaia*, taken by himself in July last on Cannock Chase; also a number of living larvæ of a species of *Eupithecia* feeding on the flower-heads of *Tanacetum vulgare*. He expressed some doubt as to the identity of the species, but the general opinion was that the larvæ were those of *Eupithecia absynthiata*.—Mr. H. Goss exhibited, for Mr. G. Bryant, a variety of the larva of *Trichiura cratægi*.—Mr. C. G. Barrett exhibited a specimen of *Plusia moneta*, Fabr., a species new to Britain, taken at Reading in July last. Mr. Goss stated that the first specimen of this species had been taken at Dover last June, and was now in the collection of Mr. Sydney Webb, of that town. Mr. Kirby said that Mynheer Snellen had reported this species as being unusually common in Holland a few years ago.—Mr. W. Dannatt exhibited a variety of *Papilio hectorides* from Paraguay. Mr. O. Salvin, F.R.S., said he had seen this form before.—Mr. C. J. Gahan exhibited a curious little larva-like creature, found in a mountain stream in Ceylon, and observed that there was some doubt as to its true position in the animal kingdom. It was made up of six distinct segments, each of which bore a single pair of laterally directed processes or unjointed appendages. Mr. Hampson remarked that the appendages were very suggestive of the parapodia of certain chætopod worms. Lord Walsingham and Mr. McLachlan expressed an opinion that the animal was of myriopodous affinities, and was not the larva of an insect.—Mr. Baker read a paper entitled "Notes on the genitalia of a gynandromorphous *Eronia hippia*."

Paris.

Academy of Sciences, October 6.—M. Duchartre in the chair.—On the determination of integrals of certain equations from partial derivatives of the second order, by M. Émile Picard.—On the balls of fire or electric globes of the St. Claude tornado, according to the report of M. Cadenat, by M. H. Faye. Prof. Cadenat, of the St. Claude College, has brought forward a number of testimonies as to the appearance of many balls of fire during the storm of August 19. It is a remarkable fact that the United States tornadoes are rarely accompanied by globular lightning discharges like those observed during the recent storms of Dreux or St. Claude. The cause may be that American tornadoes have been most frequently observed in broad daylight, whilst in France those of August 18 and 19 appeared towards the evening.—On the movement of Foucault's pendulum, by M. de Sparre. The author establishes the complete formulæ for the movement of Foucault's pendulum in air, and shows that the resistance of the air has an indirect influence on the velocity of rotation of the plane of oscillation, both diminishing the amplitude of the vibrations, and causing deformations in the curve described.—Some theorems on similar plane figures, by M. P. H. Schoute.—On a new method for testing urea, by M. M. P. Miquel.—Destruction of the tubercular virus by the products of the evaporation of certain substances, such as a mixture of alcohol and different essences, on spongy platinum, by M. Onimus.—On the fecundation of *Hydatina senta*, Ehr., by M. Maupas.—Experiments on the cultivation of wheat in a sterile siliceous soil, by M. Pagnoul. The experiments show that phosphates, especially in the soluble form, play an important *rôle* in the production of wheat; in fact, the suppression of phosphoric acid retarded the maturity of plants about ten days. The richness of the grain in nitrogenous matters increases with the proportion of nitrogen at the disposal of the plant. It it is found to decrease to 8 or 9 per cent in plants grown in soil containing no nitrogen, and reaches as much as 20 per cent—that is, much above the average—in those grown in soils in which the assimilated nitrogen was greater than that of the most fertile soils.—Observations of the part played by fluor in mineralogical syntheses, by M. Stanislas Meunier. The author finds that the introduction of fluorides renders the synthesis of labrador, nephelite, and leucite remarkably easy and rapid, and does away with the necessity for very high temperatures.

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